Violent conflict and behavior

Voors, M. J.; Nillesen, E. E. M.; Verwimp, P.; Bulte, E. H.; Lensink, B. W.; van Soest, D. P.

Published in:
American Economic Review

DOI:
10.1257/aer.102.2.941

American Economic Review 2012, 102(2): 941–964
http://dx.doi.org/10.1257/aer.102.2.941

Violent Conflict and Behavior: A Field Experiment in Burundi[†]

By Maarten J. Voors, Eleonora E. M. Nillesen, Philip Verwimp, Erwin H. Bulte, Robert Lensink, and Daan P. Van Soest*

We use a series of field experiments in rural Burundi to examine the impact of exposure to conflict on social, risk, and time preferences. We find that conflict affects behavior: individuals exposed to violence display more altruistic behavior towards their neighbors, are more risk-seeking, and have higher discount rates. Large adverse shocks can thus alter savings and investments decisions, and potentially have long-run consequences—even if the shocks themselves are temporary. (*JEL* C93, D12, D74, 012, 017, 018)

Civil wars are sometimes referred to as "development in reverse." They are typically associated with the destruction of physical capital, and temporary drops in income. This has been documented, for example, for Japan by Davis and Weinstein (2002), for Germany by Brakman, Garretsen, and Schramm (2004), and for Vietnam by Miguel and Roland (2011). A simple Solow-style growth model predicts that, following the cessation of violence, capital stocks rebound so that per capita income eventually returns to its steady state. The actual speed of recovery following conflict is the subject of debate (e.g., Chen, Loayza, and Reynal-Querol 2008; Cerra and Saxena 2008). When conflict affects institutions, social organization, or other preferences, however, it is not obvious that societies will bounce back to prewar income levels. If wars contribute to the erosion of social capital or raise levels of impatience, adverse growth and level effects could eventuate. The opposite could happen when

* Voors: Development Economics Group, Wageningen University, PO Box 8130, 6700 EW Wageningen, Netherlands and Department of Land Economy, University of Cambridge (e-mail: maarten.voors@wur.nl); Nillesen: Development Economics Group, Wageningen University, PO Box 8130, 6700 EW Wageningen, Netherlands (e-mail: eleonora.nillesen@wur.nl); Verwimp: Solvay Brussels School of Economics and Management and ECARES, and Center Emile Bernheim, Université Libre de Bruxelles, Franklin Rooseveltlaan 42, 1050 Brussels, Belgium (e-mail: philip.verwimp@ulb.ac.be); Bulte: Development Economics Group, Wageningen University, PO Box 8130, 6700 EW Wageningen, Netherlands, and Department of Economics, Tilburg University (e-mail: erwin.bulte@wur.nl); Lensink: Department of Economics, Econometrics and Finance, University of Groningen, Nettelbosje 2, 9747 AE, Groningen, Netherlands, and Development Economics Group, Wageningen University, PO Box 8130, 6700 EW Wageningen, Netherlands (e-mail: b.w.lensink@rug.nl); van Soest: Department of Spatial Economics and IVM, VU University Amsterdam, De Boelelaan 1105, 1081 HV Amsterdam, Netherlands, and Department of Economics and CentER, Tilburg University, P.O. Box 90153, 5000 LE Tilburg, Netherlands (e-mail: d.p.vansoest@uvt.nl). We thank four anonymous referees and seminar participants in Amsterdam, Bogota (Los Andes University), Frankfurt, Göttingen, Groningen, Oscarsborg, Oxford, Paris (Sorbonne), Tilburg, Wageningen, and Zürich (ETH) for helpful comments and suggestions. We are responsible for remaining errors. Gonne Beekman and Ligue Iteka provided superb assistance on this project. Nillesen was affiliated with DIW Berlin (Department of Development and Security) and ETH Zürich (Chair of Economics) during part of the research. This research was funded by grants from MICROCON (www.microconflict.eu), Groningen University, Research Foundation Flanders (FWO), the Dutch Organization for Scientific Research (NWO), grant number 452-04-333, and the Program on Evolution and Behavior.

[†] To view additional materials, visit the article page at http://dx.doi.org/10.1257/aer.102.2.941.

war invites institutional improvements or alters preferences in such a way that savings are encouraged.

A number of African countries have experienced remarkable postwar recovery after civil war. Examples include Mozambique, Angola, Rwanda, and Uganda. Undoubtedly, this is partly due to generous aid flows that typically follow the cessation of violence. Other mechanisms may also be at play, however. Heterodox social scientists have long argued that violence can carry the seed of societal reform, spurring the expansion of capitalism and promoting economic growth. For example, Cramer (2006) points to historical events to support his claim that violence can "produce institutional changes, amendments to the rule of the game. In retrospect, many changes that come to be seen as progressive have their origins in social conflicts that have taken a violent turn. Herein lies a paradox of violence and war: violence destroys but is also often associated with social creativity" (Cramer 2006, p. 279). A small literature is now emerging that appears consistent with this perspective. Blattman (2009) uses data from northern Uganda and links past abduction by rebels to increased political engagement of victims. Bellows and Miguel (2009) report positive correlations between violence and political and social behavior in Sierra Leone.

One possible interpretation consistent with this evidence is that exposure to conflict induces a shift in preferences. This explanation implies a challenge for orthodox economic theory. Economists regard preferences as exogenous and fixed in their straw man model of homo economicus (at least in the short term).[1] The notion of endogenous, or context-dependent, preferences gnaws at the foundations of standard welfare theory. Interestingly, there is little opposition to the concept of "malleable preferences" in other social sciences. Indeed, in psychology it is widely accepted that large (temporary) shocks can have persistent effects on someone's outlook on life (Carmil and Breznitz 1991; Punamäki, Qouta, and El Sarraj 1997; Tedeschi and Calhoun 2004), or in economist's terms, on someone's preferences. Since such preferences are fundamental determinants of consumption, saving, and investment behavior—the drivers of economic growth—the notion of endogenous preferences has far-reaching consequences for how we should think about development. In particular, the scope for vicious and virtuous development cycles may be radically altered.

The main objective of this paper is to examine the causal effect of exposure to violence on behavior in a series of economic experiments, in which payoffs vary between choices across three dimensions: timing, riskiness, and social consequences. Key questions are: do victims of conflict behave more prosocially, do they have a higher propensity to save and invest in the future, and are they more prone to taking risks? We try to answer these questions by pulling together survey and new experimental data from Burundi. First, we collected detailed information on the (local) history of violence in a set of Burundian communities, and on a range of household and community variables. We then conducted a series of field experiments, implementing games to determine risk, time, and social preferences in an incentive-compatible fashion. While such preferences have been measured in a variety of contexts, this

[1] In the (very) long run, evolutionary processes can change the distribution of preferences, as suggested by Netzer (2009) in the case of risk and time preferences, and by Choi and Bowles (2007) for social preferences, even if preferences are still hard-wired and fixed for individuals.

study is the first to apply experimental methods in a postconflict environment to gauge the effect of violence on human decision making.

Our results strongly suggest that exposure to violence affects behavior—possibly via altering preferences. We find that individuals who have either experienced violence themselves, or who live in communities that have been violently attacked, display more altruistic behavior, are more risk-seeking, and act less patiently. Results are robust across several specifications, and are obtained for both experimental data as well as observational data collected in the survey (information about social capital, crop choice, and expenditures on farm improvement). We believe these findings shed new light on postwar recovery processes by speaking against overly pessimistic views on the destructive long-term consequences of civil war.

Identifying whether or not preferences are endogenous poses two problems for the analyst. The first concern regards the potential endogeneity of the shock due to selection bias and nonrandom attrition. Although our tests indicate that attrition is unlikely to be substantial, we probe the robustness of our findings using various additional analyses, including an instrumental variable approach. Second, we cannot directly observe preferences. Instead, we observe behavior (in an experiment), and can try to make inferences about the underlying preferences. While the literature commonly interprets experimental play as reflecting underlying preferences (e.g., Henrich et al. 2001; Tanaka, Camerer, and Nguyen 2010), behavior and preferences are not identical concepts. Behavior is affected by many factors, including the (social) context and beliefs about the behaviors of others (which may not be invariant with respect to the history of violence either). We present these competing hypotheses, and point out that our quasi-experimental data do not allow us to separate cleanly the relative importance of the various mechanisms linking violence to shifts in behavior.

This paper is organized as follows. In Section II we discuss the background to the conflict. In Sections II and III we describe our data and research design. Section IV discusses our identification strategy, including the way we address endogeneity issues. In Section V we present our main experimental results, including a robustness analysis, and aim to interpret them in the context of economic thinking. Section VI concludes.

I. Background to the Conflict in Burundi

Since independence, Burundi has been the stage of nearly three decades of civil war between the country's two main ethnic groups: Hutu (85 percent of the population) and Tutsi (14 percent). At the outbreak of the most recent episode of violence in 1993, following the assassination of the country's first Hutu president, Melchior Ndadaye, Hutu groups (mostly farmers) targeted Tutsi in retaliation throughout the country, killing thousands of Tutsi within weeks. In turn, the Tutsi-dominated army responded with indiscriminate and large-scale attacks on Hutu, " … making no distinction between communities which had been involved in violence against Tutsi and those that were not" (Human Rights Watch 1998, p. 15).

This started a civil war. In the Northern provinces, benefiting from the proximity of the Congolese border and the Kibira forest for shelter, several rebel groups formed (initially the Conseil Nationale pour la Défense de la Démocratie, but later others followed). Much of the war was concentrated near the nation's capital, Bujumbura, as both

rebels and the army fought over its control, but the conflict soon spread to the northeastern provinces, and later to central and southern Burundi (for information about the evolution of the war, refer to United Nations 1996; Bundervoet, Verwimp, and Akresh 2009; and Chrétien and Mukuri 2000). Eventually, rebel groups and the army ravaged communities throughout the country, but a gradient in the intensity of violence is evident—fighting was more intense near the capital.

Which factors explain patterns of violence in Burundi? Standard explanations of greed and grievance appear to have little explanatory power (see Section V below). Instead, Uvin (1999) proposes an explanation based on fear. Traumatized by earlier waves of violence in the 1970s, Hutu were fearful of being victimized (again) and launched preemptive attacks on Tutsi army barracks or Tutsi civilians. The Tutsi-dominated army responded by killing Hutu indiscriminately. In response, attacks by Hutu militia became "increasingly brutal and random, affecting all of the country and causing profound fear among Tutsi as well as Hutu bystanders" (Uvin 1999, p. 262). Chaos and anarchy erupted, and the civilian population paid a high prize. Human Rights Watch refers to most of the victims of violence in Burundi as "proxy targets" (Human Rights Watch 1998): while the army and Hutu militias fought for control, direct clashes between the rival fractions were rare; instead most of the violence was unleashed on civilians.

Violence was largely indiscriminate because of the army's inability to identify rebels, but also by a desire for extermination, "revenge by proxy," plundering, and a perceived need to demonstrate power as part of the tactics of fear to control a population (Uvin 1999; Krueger and Krueger 2007; for a broader discussion of such tactics, see Kalyvas 2006). This type of violence is near-exogenous to household characteristics and local economic conditions, hitting communities and civilians regardless of social status, education, or income. Both army and rebels arbitrarily and sweepingly raided communities throughout the country.[2] People were also attacked while fleeing or during the creation of "regroupment camps," between 1996 and 2001. In areas suspected of rebel activity, the army led a brutal campaign to cut off rebel supplies, support, and shelter. Civilians were evicted from their homes and forcibly brought together in camps. As an incident report from Human Rights Watch states: "[… S]oldiers would … order people to gather at a specific site. They killed anyone who refused" (Human Rights Watch 1998, p. 29). At its apex, an estimated total of 220,000 people lived in these camps.

Burundi has only recently started to recover from this violence, which left over 300,000 Burundians dead and displaced 1.2 million people (Ngaruko and Nkurunziza 2000).

II. Research Design and Data

We conducted our series of experiments in March and April 2009, using a sample of 300 household heads from 35 communities in rural Burundi. These communities and households were drawn from a set of 100 communities that were visited earlier

[2] Unlike the situation in neighboring Rwanda, where conditions of anarchy enabled villagers to settle scores and target specific fellow villagers (e.g., André and Platteau 1998), there is little or no evidence of such selective killings in Burundi (Uvin 1999).

in 1998 and 2007 to collect survey data. The 2007 survey included variables on local history of conflict, social capital, and a range of household and community variables (including proxies for social, risk, and time preferences).[3] We randomly selected 35 communities and revisited all respondents of the earlier survey, inviting them to participate in a series of experiments. Of the 35 communities, 24 experienced violence in the period 1993–2003, and 11 were not exposed to violence.[4]

The key variable of interest is our community measure of conflict victimization measured as the share of war-related deaths (1993–2003) among the total population, resulting from confrontations between the army and rebels or one-sided violence by either group.[5] Such attacks resulted in the death of up to 15 percent of the communities' inhabitants. We also create a household-level victimization index, based on the experience of death, theft, ambush, forced labor, and torture of household members. The index is additive and, given its arbitrary scale, normalized to have mean 0 and standard deviation 1 (Table 1, Panel B). A detailed description of our variables and their sources is provided in an online Appendix.

III. Experimental Games

For our experiments we adapted well-established experimental game protocols to implement social orientation, risk, and time preference experiments. These experiments are discussed in more detail in an online Appendix; to economize on space we highlight only key features here.

To measure social preferences we used a modified version of the social value orientation experiment devised by Liebrand (1984). Subjects were anonymously matched to another participant from their community (their "partner"), and made six choices between two own-other payoff combinations, A and B. These combinations differed not only in the proposed division of the sums of money but also in the total sums to be allocated. We conducted the experiment offering nonnegative payments only.[6] A subject's social orientation is reflected by the ratio of the total amounts of money allocated to the partner and to himself. This ranges from totally selfish (if the subject always chose the allocation with the highest payment

[3] The data collection was a collaborative effort between the Institut de Statistiques et d'Etudes Economiques du Burundi (ISTEEBU), Antwerp University, Vrije Universiteit Brussel, and Wageningen University, and was implemented under the flag of MICROCON—an EU-funded project focusing on household analysis of violent conflict in various regions of the world. A first wave of data was collected by the World Bank halfway through the war (in 1998) in 391 communities. For a total of 1,400 households in 100 communities we have panel data (1998–2007) regarding many important household characteristics as well as information on the development and consequences of armed conflict. The surveys and their sampling designs are described in an online Appendix.

[4] The end of the conflict was officially sealed in 2005 when a new Constitution, largely based on the Arusha Peace Agreement, was approved by referendum. Yet, the intensity of conflict in the last 2 years was negligible as compared to the intensity in the first 11 years. When constructing our variables on conflict and victimization, we focus on the incidents in the 1993–2003 period.

[5] Reports of violence are based on retrospective accounts by villagers (collected in village focus group meetings) and cross-checked with published reports from ACLED database of PRIO (Raleigh et al. 2010).

[6] Because we only offered choices with nonnegative payoffs for each participant, our experiment does not confound altruism and competitive preferences—unlike the standard Liebrand (1984) design. Our design does conflate altruism and a preference for efficiency, though, in three of the six choices subjects are asked to make. Moreover, even though this game is nonstrategic (it is very similar to a dictator game), subjects may base their decisions on their expectations regarding the decisions made by the anonymous village members they are matched with—as pointed out by a referee. Unfortunately, we did not inquire into the expectations with respect to the play of others, so we cannot exclude that differences in beliefs about the play of others may also drive part of the result.

Table 1—Descriptives

	Observations household level	Observations community level	Mean	SD	Min	Max
Panel A. Preferences						
Social preferences (0–100, 2009)	286	35	27.32	27.22	0	100
Risk preferences Gains (2009)	220	35	1.87	1.31	0	3
Risk preferences Losses (2009)	233	35	2.31	1.18	0	3
Discount rate (percent, 2009)	273	35	40.16	41.43	0	100
Panel B. Conflict variables						
Relative number of dead in attacks (percent, 1993–2003)[3]		35	1.99	4.09	0	15.63
Attack (1993–1998)[8]		35	0.69	0.47	0	1
Individual victimization index (1993–2003)[2]	287	35	0	1	−0.78	5.03
Physical attack (1993–2003)[2]	287	35	0.32	0.47	0	1
Nonphysical attack (1993–2003)[2]	287	35	0.45	0.50	0	1
Panel C. Household variables						
Household head is literate (1998)[1]	285	35	0.40	0.49	0	1
Household head is literate (2009)[4]	287	35	1.60	0.49	1	2
Household head age (1998)[1]	283	35	42.28	15.30	16	99
Household head age (2009)[4]	286	35	45.96	15.11	18	90
Household head is male (1998)[1]	286	35	0.62	0.49	0	1
Household head is male (2009)[4]	287	35	0.79	0.41	0	1
Household head ethnic origin (2009)[4]	287	35	0.22	0.41	0	1
Total expenditures (1998)[1]	287	35	8.56	0.70	5.17	10.7
Total land holdings per capita (ha^2, 2007)[2]	281	35	5.54	5.13	0.10	34.25
Household head is livestock farmer (1993)[4]	287	35	0.41	0.49	0	1
Perceived trust level (1998)[2]	287	35	4.64	2.17	1	10
Social capital index (2007)[2]	282	35	0.02	1.04	−0.47	4.49
Investments farm buildings (FBU, 1998)[1]	241	35	5.57	1.20	1.08	8.52
Investments farm buildings (FBU, 2007)[2]	287	35	229.35	1,022.67	0	12,155.87
Share of cash crops in total production (1998)[1]	276	35	0.22	0.30	0	1
Share of cash crops in total production (2007)[2]	280	35	0.06	0.14	0	1
Severe draught (percent yes, 2007–2009)[4]	277	35	0.33	0.47	0	1
Access rain (percent yes 2007–2009)[4]	231	35	0.62	0.49	0	1
Manioc disease (percent yes 2007–2009)[9]	287	35	0.40	0.49	0	1
Upcoming ceremony (percent yes, 2009)[4]	231	35	0.39	0.49	0	1
Panel D. Community variables						
Land Gini coefficient (2007)[2]	35	0.29	0.20	0	0.54	
Distance to market (2007)[3]	35	2.86	0.72	1.38	4.22	
Conflict over land (percent yes, 2007)[2]	35	0.25	0.15	0	0.6	
Ethnic homogeneity (1993)[5]	31	82.87	16.89	30	99	
Ethnic homogeneity (2009)[5]	35	86.68	15.57	30	100	
Votes for Ndadaye in 1993 (percent)[7]	34	64.51	17.68	9.94	93.51	
Socioeconomic homogeneity (1998)[2]	35	1.66	1.06	1	5	
Socioeconomic homogeneity (2007)[2]	35	1.57	0.95	1	5	
Population density (log, 1990)[5]	35	5.43	0.51	4.20	6.11	
Population density (log, 2008)[6]	35	5.76	0.47	4.50	6.49	
Per capita total expenditure (log, 2007)[2]	35	9.25	0.47	8.09	10.40	
Distance to Bujumbura (km, log)	35	4.49	0.39	3.68	5.12	
Altitude (m, log)[5]	35	7.41	0.10	7.14	7.70	

Sources: (1) Burundi Priority Household Survey 1998, (2) Burundi Priority Household Survey 2007, (3) Burundi Community Survey 2007, (4) Burundi Experiments Exit Survey 2009, (5) Monographies Communales Burundi. Ministre de la Planification du Développement et de la Reconstruction Nationale, Bujumbura, 2006 (6) Recensement National de la Population et de l'Habitat 2008, (7) Sinunguruza T. 2001. Les Elections au Burundi. Tout Savoir et Tirer des Leçons de Juin, (8) Raleigh et al. 2010, (9) FAO Valuation Des Recoltes, Des Approvisionnements Alimentaires et de la Situation Nutritionelle, 2006.

for himself) to totally altruistic (if he always chose the option with the highest payment for his partner). We rescaled the results such that social orientation is measured on a scale from 0 to 100, with 0 denoting purely selfish preferences, 100 identifying the subject to be maximizing his partner's payoff, and 50 identifying the social optimum (i.e., choosing allocations to maximize joint payoffs). On average we find a value of 27, indicating that most subjects are fairly individualistic (Table 1, Panel A).[7]

Risk preferences were measured using a game based on Harbaugh, Krause, and Vesterlund (2002) where subjects could choose between playing a simple gamble and receiving a specific amount of money with certainty. Six choice cards were presented, each of which offered them the choice between A, receiving (or losing) an amount of money with certainty, and B, participating in a game where they could either gain (lose) 2,000 FBU[8] with probability 0.3, or gain (lose) nothing with probability 0.7.[9] Hence, the expected absolute value of the gamble was always the same, and the amount of money received with certainty varied across choices (lower, equal to, and higher than the expected value of the gamble). The point at which a subject switches from the risky to the safe alternative allows us to determine the respondent's degree of risk aversion.

To measure time preferences, we presented subjects with a set of nine simple pairwise choices between two options: receiving an amount of money at some date in the near future, and receiving a larger sum at a later time. The two options to choose between were A, receive 1,000 FBU the following day, and B, receive 1,000 $(1 + d)$ FBU in two weeks plus one day, with d equal to 0.00, 0.01, 0.02, 0.05, 0.10, 0.40, 0.70, and 1.00. Subsequently, at the highest interest rate subjects earned an additional 1,000 FBU by waiting two weeks. In the experiment, subjects were asked to identify the smallest d for which they preferred B to A—the earlier people switch from A to B, the more patient they are. The fact that there has been no war-related violence in our study area for several years combined with the relatively short delay (two weeks only) implies we believe that our estimate of time preferences is not confounded by risk due to anticipated violence—people are unlikely to choose the near immediate pay-off because they fear being killed by violence in the next two weeks.

After the series of experiments, the average participant walked away with a sum that is the equivalent of five days of wages for unskilled labor in Burundi—a salient incentive.

IV. Identification Strategy, Exogeneity, and Selection Bias

The key assumption underlying our empirical approach is that violence across and within communities was exogenous with respect to individual preferences. Any covariation of preferences and exposure to violence may, however, be due to

[7] This experiment measures the weight placed on another subject's welfare, but it may be an imperfect predictor of cooperation in the field. For example, patience may matter too—prosocial behavior in a fishery requires foregoing higher returns today to increase the community's future returns (e.g., Fehr and Leibbrandt 2008).

[8] USD 1 = 1,210 FBU (May 20, 2009), which is roughly equal to a full day's wage rate for unskilled labor.

[9] Our design differs slightly from Harbaugh, Krause, and Vesterlund (2002) as we specifically use information from questions where the certainty equivalent is different from the expected value of the gamble.

TABLE 2—EXOGENEITY

Dependent variable	Attack during 1993–2003	Percentage dead in attacks 1993–2003	Percentage dead in attacks 1993–2003	Physical attack on household member	Nonphysical attack on household member	Present village in 1998 and 2009	Present in village in 1993 and 1998
	Probit (1)	OLS (2)	OLS (3)	Probit (4)	Probit (5)	Probit (6)	Probit (7)
Percentage literate household heads (in 1998)	−0.0282 [0.0208]	−0.0561 [0.041]	−0.002 [0.001]				
Average age household head (in 1998)	−0.0273 [0.0520]	0.0625 [0.133]	−0.001 [0.005]				
Percent male (in 1998)	−0.00170 [0.0248]	−0.0587 [0.0634]	0.003 [0.002]				
Percentage livestock farmers (in 1993)	0.0252 [0.0218]	0.0524 [0.0510]					
Density in 1990 (log)	−2.119 [1.443]	0.0317 [1.970]	0.081 [0.123]				
Ethnic homogeneity (in 1993)	0.0155 [0.0235]	0.0200 [0.0557]					
Socioeconomic homogeneity (in1998)	−0.295 [0.392]	−0.934 [0.872]	0.051 [0.033]				
Percentage of votes for Ndadaye (in 1993)	0.0227 [0.0283]	0.0457 [0.0575]					
Distance to Bujumbura (km, log)	−2.561 [1.536]*	−4.884 [3.039]+	0.109 [0.189]				
Altitude (m, log)	−8.112 [5.420]+	−16.09 [8.252]*	−0.748 [0.303]**				
Relative number of dead in attacks 1993–2003						0.0255 [0.0345]	
Number of attacks 1993–1998							−0.001 [0.002]

(*Continued*)

(i) nonrandom (or targeted) violence or (ii) nonrandom attrition in the sample. We present evidence to suggest that violence was not very targeted, and that attrition bias is not likely to affect our results.

Regarding the first, there is ample evidence highlighting the brutal and indiscriminate nature of the Burundi conflict (Human Rights Watch 1998; Uvin 1999; Krueger and Krueger 2007). To statistically examine whether "selection into violence" biases our results, we follow an approach taken by Bellows and Miguel (2009), and first assess whether violence experienced by communities is associated with lagged community characteristics; see Table 2. Violence is measured as whether or not the village was attacked during 1993–2003 (column 1), and as the number of people dying in attacks in that period, expressed as a share of the total population (column 2). Our specification tests implicitly for two theories of violence: greed and grievance (see Collier et al. 2003). The first is "economic profit"—exploiting the opportunity to use violence for stealing the assets of others, including livestock. The second is that violence was driven by grievance, or perhaps ethnic considerations, as measured by ethnic homogeneity, votes for the assassinated Hutu president Ndadaye, and socioeconomic homogeneity. In these two columns we find no support for the hypothesis that victims have been selected because of either motivation. The only two (exogenous) variables correlated with the share of villagers killed are geographical

TABLE 2—EXOGENEITY (*Continued*)

Dependent variable	Attack during 1993–2003	Percentage dead in attacks 1993–2003	Percentage dead in attacks 1993–2003	Physical attack on household member	Nonphysical attack on household member	Present village in 1998 and 2009	Present in village in 1993 and 1998
	Probit (1)	OLS (2)	OLS (3)	Probit (4)	Probit (5)	Probit (6)	Probit (7)
Respondent is literate (in 1998)				−0.114	0.224	0.312	0.023
				[0.251]	[0.234]	[0.208]	[0.137]
Respondent age (in 1998)				−0.005	0.006	−0.002	0.012
				[0.008]	[0.008]	[0.006]	[0.003]***
Respondent is male (in 1998)				0.447	−0.248	−0.794	−0.054
				[0.303]	[0.278]	[0.225]***	[0.083]
Livestock farmer in 1993				0.228	0.0958	0.0660	−0.0003
				[0.230]	[0.214]	[0.226]	[0.084]
Respondent ethnicity				−0.498	−0.473		
				[0.310]	[0.296]		
Total expenditures (in 1998)				0.361	0.345	−0.193	
				[0.222]	[0.217]	[0.170]	
Perceived trust level (in 1998)				−0.001	0.028		
				[0.051]	[0.046]		
Share of cash crops in total production (in 1998)				0.176	−0.499	−0.097	
				[0.484]	[0.434]	[0.367]	
Expenditures farm improvement (in1998)				0.0949	0.237	0.030	
				[0.171]	[0.161]	[0.098]	
Constant	83.37	142.5	4.784	−10.06	−4.199	2.295	−0.993
	[52.05]	[74.01]*	[2.473]*	[0.00]	[1.951]*	[1.448]	[0.215]***
FE	No	No	Yes (province level)	Yes (village level)	Yes (village level)	Yes (stratum level)	Yes (province level)
N	30	30	94	198	219	279	1,766
Adjusted R^2	0.33	0.19	0.27	0.19	0.16	0.10	0.11

Notes: Included fixed effects are at lowest level possible. Columns 1 and 2 use 30 observations instead of 35 due to missing 1993 ethnicity data. Column 3 uses 94 observations and not 100 due to missing 1993 population data. Dependent variable dead in attacks relative to number of households and not population due to missing population data. Columns 1–6 use mainly BPHS and BSC 2009 data. Ethnic livestock ownership data are from BEES (2009). Dependent variable in column 6 is a dummy, 1 if respondent was interviewed in both 1998 and 2007, zero else. Literacy is measured as years of education. Column 7 uses the ESD-SR 2002 data to assess attrition between 1993 and 1998. Dependent variable is a dummy, 1 if respondent was present in village in 1993 and 1998, zero else. Literacy, age, and gender are measured in 2002; the dummy for livestock farmer in 1993 is based on recall in 2002, the number of attacks between 1993 and 1998 were drawn from the ACLED database and matched to the UNFPA data at the commune level. The proportion of people absent from the sample was 16 percent. Regression uses province fixed effects. Including village fixed effects reduces the number of observations but does not change the results. Standard errors in brackets.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.
+ Significant at the 15 percent level.

variables—distance to Bujumbura, the nation's capital, and remoteness (proxied by altitude)—consistent with the pattern of violence in Burundi as sketched by Uvin (1999, see Section II). The F-tests on the joint significance of all nongeographical variables in columns 1 and 2 yield *p*-values greater than 0.30.

These analyses are based on a relatively small number of observations—because of missing variables, only 30 villages are used in the regressions. Hence we potentially face the risk of incorrectly failing to reject the null of nontargeted violence. We tested the probability of a type II error in column 2 of Table 2. When focusing on the explanatory power of the nongeographical variables, we find that this probability is

equal to 0.38 (or the power is 0.62).[10] Fortunately, we can raise the power of these regressions by applying a similar analysis to the larger dataset of 100 villages. The results are reported in column 3, and the probability of a type II error is now less than 0.14 (power is 0.86), which is below the 0.20 (above 0.80) threshold routinely assumed in empirical analysis. As the 35 villages in which we ran our experiments were randomly drawn from the set of 100 villages we used in our 2007 survey and the regression results in columns 2 and 3 are qualitatively identical, we conclude that the probability of incorrectly maintaining the null of nontargeted violence is acceptably small. Hence, our data provide support for the anecdotal evidence that violence in the Burundi war was not very targeted. Still, in what follows we will use altitude and distance to Bujumbura as instruments for violence to attenuate any remaining endogeneity concerns and measurement error.

In columns 4 and 5 of Table 2 we repeat the analysis of columns 1 and 2, but now at the household level. We have two proxies for household exposure to violence: (i) whether a physical attack happened to a household member, and (ii) whether household members were exposed to nonphysical violence (including theft, forced labor, etc.). Neither variable is correlated with a range of household characteristics (income, gender, literacy, etc.), which is in line with anecdotal accounts of violence in Burundi (Human Rights Watch 1998; Krueger and Krueger 2007). The same applies to 1998 survey-based data related to the preferences we are interested in: (i) perceived trust levels (a measure of social capital as a proxy for social preferences); (ii) crop choice (proxy for risk preferences); and (iii) expenditures on farm improvements (proxy for time preferences; see Section VB).[11] The regression models in columns 4 and 5 are powered adequately as the probability of a Type II error is less than 0.01 in both instances. While we do not find evidence that (non)trusting individuals have a higher probability of falling victim to violence,[12] we obviously cannot rule out that certain noncooperative individuals were targeted during the violence (note we lack prewar evidence of the villagers' experimental play).

In columns 6 and 7 we analyze potential nonrandom attrition between 1993 and 2009.[13] In column 6 we analyze whether a nonrandom subset of the 1998 population was absent when we invited them for the experiments in 2009. This is important as households that migrate in anticipation of violence may have different preferences than those households that stayed behind. We follow the approach taken by Fitzgerald, Gottschalk, and Moffitt (1998) and estimate a probit model of 1998–2009 attrition on a range of 1998 household characteristics. All but one variable enter nonsignificantly: households headed by a male were less likely to be present in the 2009 sample (we return to this below). But if our 1998 data were drawn from a nonrandom subset of the 1993 population, this might have consequences for the

[10] We used G*Power3 software to conduct the power tests; see Faul et al. (2007).

[11] We lack prewar data for these variables. Instead we rely on early war data from our first survey wave, conducted in 1998.

[12] We find additional support for this claim when testing whether the vectors of individually insignificant variables in Table 2 are jointly insignificant. The *p*-values obtained (using F-tests for the OLS models and Likelihood Ratio tests for the probit models) are all larger than 0.10—and many are greater than 0.30. These results are consistent with the results of the power tests presented, and indicate we cannot reject the null hypothesis (i.e., the vector of individually insignificant variables is also jointly insignificant).

[13] Attrition between our first survey in 1998 and the second wave in 2007 is below 14 percent, and below 1 percent between this second wave and our experiments in 2009. The 1998–2007 attrition level is modest given the length of the period and the circumstances of civil warfare.

external validity of our findings. We use another dataset to explore the magnitude of nonrandom attrition between 1993 and 1998. In 2002, UNFPA-Burundi undertook a nationwide Demographic and Health Survey (ESD-SR). In the survey respondents were asked, among other questions, to list their entire migration history, starting in January 1993 (before the start of the civil war). This data hence allows us to explore sources of attrition (which was below 16 percent) over the 1993–1998 period. In column 7 of Table 2 we present the results of an attrition model in which we include a range of household controls and fixed effects. We find that only age appeared to matter for migration decisions for this sample; older people were more likely to have stayed behind than younger heads of households—and significantly so at the 1 percent level. We return to these issues when testing the robustness of our results in Section VB.

V. Conflict, Behavior, and Preferences

The descriptive statistics in Panel A of Table 1 suggest considerable heterogeneity in experimental behavior. In this section we investigate whether experimental behavior varies with exposure to conflict, and regress decisions made in the social-, risk-, and time-preference experiments on our measures of violence. We include several household and community characteristics as controls, and also include regional fixed effects.[14] We focus primarily on the relationship between *community* exposure to violence and *individual* preferences. The reason is that even if only a subset of individuals directly experienced acts of violence, the consequences may be felt throughout the community (Yehuda 2002). Our main measure of violence is the total number of dead during 1993–2003 relative to population size in the community. In some models, however, we also include an index of individual exposure to violence. Throughout, we cluster standard errors at the community level to account for intracommunity correlation.

A. Conflict and Behavior

We explore the relationship between conflict and behavior in the experimental games in Tables 3, 4, and 5.[15] In Table 3 we report the results for our measure for prosocial behavior. Across all OLS specifications we record a statistically significant and positive correlation between altruistic behavior and conflict intensity at the community level (column 1) as well as at the household level (columns 2–5). This is in line with survey work by Bellows and Miguel (2009), who report an increase in social cohesion and political participation in response to violence.

Our results are robust to the inclusion of ethnicity fixed effects, as well as a series of other household and community controls and regional fixed effects (Table 3, columns 3 and 4, respectively).[16] For example, behavior in the experiment is more

[14] See the online Appendix for the exact variable definitions.

[15] All regressions in Tables 3–5 use OLS or 2SLS. As our dependent variables take only a limited number of values, we also estimate the models in columns 1–6 using an ordered probit specification. The results are qualitatively identical and available on request.

[16] Note that the coefficient on violence increases if controls are included. Following the reasoning of Bellows and Miguel (2009) this suggests that it is unlikely that omitted variable bias explains away the conflict effect (see also Altonji, Elder, and Taber 2005).

Table 3—Conflict and Social Preferences

	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	2SLS (6)	2SLS (7)
Percentage dead in attacks	1.073 [0.489]**	0.875 [0.460]*	1.688 [0.464]***	1.686 [0.523]***		2.892 [0.958]***	0.486
Individual victimization index					2.940 [1.745]*		
Respondent is literate			9.550 [3.521]***	9.430 [3.450]***	6.645 [3.945]*	10.39 [3.418]***	0.190
Respondent age			−0.207 [0.108]*	−0.201 [0.109]*	−0.330 [0.158]**	−0.202 [0.110]*	−0.111
Respondent is male			4.204 [3.373]	4.662 [3.436]	10.97 [4.234]**	5.615 [3.174]*	0.099
Total land holdings per capita			0.807 [0.253]***	0.730 [0.250]***	0.590 [0.380]	0.827 [0.263]***	0.152
Land Gini coefficient			−16.47 [10.01]	−13.60 [9.376]		−20.66 [12.12]*	−0.150
Distance to market			−6.012 [2.583]**	−6.020 [2.542]**		−7.633 [2.851]***	−0.198
Conflict over land			−29.99 [11.65]**	−26.79 [10.74]**		−36.77 [13.32]***	−0.207
Ethnic homogeneity			0.204 [0.118]*	0.232 [0.124]*		0.148 [0.126]	0.080
Socioeconomic homogeneity			−2.387 [1.948]	−2.245 [2.154]		−2.244 [2.077]	−0.072
Population density			6.712 [4.173]	5.770 [5.395]		8.511 [6.074]	0.136*
Per capita total expenditure			1.995 [3.046]	2.610 [3.845]		0.947 [4.483]	0.016
Constant	23.688 [2.569]***	25.20 [2.296]***	−23.68 [42.45]	−24.86 [42.63]	4.33 [34.16]	−12.17 [44.99]	
FE	No	No	No	Yes (stratum level)	Yes (village level)	Yes (stratum level)	Yes (stratum level)
1998 household controls	No	No	No	No	Yes	No	No
N	35	286	278	278	225	278	278
Adjusted R^2	0.099	0.018	0.144	0.140	0.212	0.166	0.166
First stage instruments							
Distance to Bujumbura (log)						−6.687 [2.489]***	
Altitude (log)						−21.988 [9.211]***	
Hansen J, *p*-value						0.51	
Partial F						7.30	

Notes: Dependent variable: degree of altruism scale 0–100. Robust standard errors in brackets are clustered at community level. Sampling weights are not included, but results are qualitatively identical. Column 1: dependent variable is community average. Column 5: household 1998 controls included. Column 6: excluded instruments of first stage reported only. The beta coefficients of column 6 are reported in column 7. Estimations in column 7 do not include clustered standard errors.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

prosocial when respondents are literate and own more land. We find weak evidence that social behavior is declining in age. Turning to the community-level controls, social behavior is positively associated with ethnic homogeneity, and negatively associated with distance to the market and ongoing conflicts within the community over land. The effect of market integration is consistent with Henrich et al. (2001), and the land conflict and ethnicity outcomes make intuitive sense. We find that population density, average community income, and land distribution are not correlated with social behavior.

Next, we employ a household-level conflict variable, rather than a community-based measure (Table 3, column 5). We again find a positive correlation with social behavior, indicating that both individual- and community-level violence are associated with altruistic behavior. Note that here, where we use village-level fixed effects, we essentially compare victims and nonvictims within one village. Compared to the results presented in columns 1–4 we now also find that male respondents are slightly more inclined to be generous to (nonkin) fellow community members—all else equal.

OLS regression results for risk preferences are presented in Table 4. Throughout, we observe a positive correlation between community-level conflict intensity and risk seeking.[17] This result is obtained when focusing on the community as well as on the individual level (column 1 versus column 2), and is robust to including common controls and fixed effects (columns 3–4). In column 6 the dependent variable measures preferences over losses. One key insight from the seminal paper by Kahneman and Tversky (1979) is that people value changes in gains and losses differently. Their work resonates in ours because we find that conflict induces risk seeking over gains (column 4) while it does not affect attitudes towards losses (column 6). Though work by economists on shocks and risk preferences has so far been limited, this result suggests it may be a viable area for future research. It is striking to observe that risk preferences are not (robustly) associated with any of the household or community-level controls.

Finally, in Table 5 (columns 1–5) we summarize the impact of conflict on intertemporal choices. The models suggest that exposure to conflict causes an increase in discount rates. While the evidence seems more mixed than for the other experiments, violence appears to make people less patient. It is interesting to note that time preferences are not associated with any of the household-level variables (including individual exposure to violence, when controlling for community attacks). In contrast, several of the community variables enter significantly. Communities with higher levels of ethnic homogeneity and/or more unequal land holdings display lower discount rates.

[17] Andreoni and Sprenger (forthcoming) document that separate utility functions govern the assessment of certain and uncertain payoffs. Their results indicate that experiments like ours conflate the curvature of the utility function and a so-called "certainty effect" as they find that subjects exhibit a strong preference for payoffs that are certain. We admit that we cannot distinguish between these two possibilities, but the conclusion remains the same—exposure to conflict makes people more prone to take risks as their evaluation of the payoffs is affected. Note that our measure of time preference is not affected as respondents are asked to choose between two equally (un)certain payoffs—delayed payment by a trusted organization (to be discussed next)—in which case expected utility theory applies according to Andreoni and Sprenger (forthcoming).

TABLE 4—CONFLICT AND RISK PREFERENCES

	Gains	Gains	Gains	Gains	Gains	Losses	Gains	Gains
	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)	2SLS (7)	2SLS (8)
Percentage dead in attacks	0.078 [0.024]***	0.0651 [0.0246]**	0.0527 [0.0246]**	0.0634 [0.0263]**		0.0196 [0.0204]	0.0729 [0.0376]*	0.258
Individual victimization index					0.165 [0.114]+			
Respondent literate			−0.225 [0.179]	−0.227 [0.183]	−0.005 [0.274]	−0.151 [0.182]	−0.214 [0.170]	−0.083
Respondent age			0.005 [0.006]	0.006 [0.006]	0.001 [0.009]	−0.008 [0.006]	0.006 [0.005]	0.065
Respondent is male			−0.217 [0.188]	−0.186 [0.188]	−0.279 [0.256]	0.0537 [0.141]	−0.176 [0.165]	−0.067
Total land holdings per capita			−0.025 [0.018]	−0.0341 [0.018]*	−0.028 [0.025]	−0.010 [0.019]	−0.034 [0.017]*	−0.130
Land Gini coefficient			−1.129 [0.745]	−1.026 [0.725]		0.0393 [0.731]	−1.092 [0.740]	−0.170
Distance to market			0.124 [0.129]	0.137 [0.122]		0.122 [0.141]	0.125 [0.129]	0.070
Conflict over land			0.0205 [0.960]	0.254 [0.918]		0.510 [0.877]	0.167 [1.045]	0.019
Ethnic homogeneity			−0.000878 [0.00641]	−0.0000909 [0.00671]		0.00161 [0.00877]	−0.001 [0.006]	0.009
Socioeconomic homogeneity			−0.00605 [0.115]	0.0314 [0.117]		−0.0381 [0.136]	0.0313 [0.111]	0.022
Population density			0.326 [0.225]	0.418 [0.332]		0.430 [0.292]	0.441 [0.333]	0.149
Per capita total expenditure			−0.249 [0.143]*	−0.274 [0.170]		−0.0302 [0.239]	−0.290 [0.177]	−0.106
Constant	1.680 [0.131]***	1.716 [0.145]***	2.237 [1.986]	2.022 [2.116]	4.902 [1.869]**	0.00443 [2.607]	2.149 [2.045]	
FE	No	No	No	Yes (stratum level)	Yes (village level)	Yes (stratum level)	Yes (stratum level)	Yes (stratum level)
1998 household controls	No	No	No	No	Yes	No	No	No
N	35	220	213	213	175	228	213	213
Adjusted R^2	0.17	0.05	0.18	0.20	0.38	0.09	0.20	0.20
First stage instruments								
Distance to Bujumbura (log)							−5.499 [2.525]*	
Altitude (log)							−19.221 [8.474]**	
Hansen J, *p*-value							0.10	
Partial F							6.50	

Notes: Dependent variable ranges from 0 (risk averse) to 3 (risk loving). Robust standard errors in brackets are clustered at community level. Sampling weights are not included, and results are qualitatively identical. Column 1: dependent variable is community average. Column 5: household 1998 controls included. Column 7: excluded instruments of first stage reported only. Column 8: beta coefficients of column 7. Estimations in column 8 do not include clustered standard errors.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.
+ Significant at the 15 percent level.

TABLE 5—CONFLICT AND TIME PREFERENCES

	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	2SLS (6)	2SLS (7)
Percentage dead in attacks	0.543 [0.519]	0.666 [0.467]	1.197 [0.624]*	1.188 [0.575]**		2.337 [1.058]**	0.265
Individual victimization index					2.826 [3.294]		
Respondent literate			−5.540 [5.594]	−5.051 [5.610]	−12.51 [6.463]*	−4.136 [5.405]	−0.050
Respondent age			−0.162 [0.189]	−0.174 [0.186]	−0.254 [0.267]	−0.180 [0.178]	−0.065
Respondent is male			−0.168 [4.921]	−1.225 [4.747]	−11.42 [7.483]	−0.382 [4.470]	−0.004
Total land holdings per capita			0.154 [0.442]	0.331 [0.439]	−0.516 [0.633]	0.423 [0.408]	0.053
Land Gini coefficient			−32.65 [23.40]	−39.88 [22.50]*		−47.18 [21.50]**	−0.230
Distance to market			3.013 [3.704]	2.873 [4.211]		1.318 [4.769]	0.023
Conflict over land			−14.49 [24.33]	−22.92 [22.57]		−33.35 [22.80]	−0.124
Ethnic homogeneity			−0.432 [0.224]*	−0.496 [0.212]**		−0.582 [0.195]***	−0.207
Socioeconomic homogeneity			7.287 [4.516]	6.839 [4.928]		6.711 [4.881]	0.144
Population density			7.489 [6.715]	9.804 [7.421]		12.48 [7.999]	0.132
Per capita total expenditure			8.874 [6.479]	7.632 [5.267]		5.825 [5.564]	0.066
Constant	39.38 [4.377]***	38.49 [3.950]***	−44.73 [72.99]	−45.15 [70.65]	74.50 [64.10]	−29.89 [71.18]	
FE	No	No	No	Yes (stratum level)	Yes (village level)	Yes (stratum level)	Yes (stratum level)
1998 household controls	No	No	No	No	Yes	No	No
N	35	273	266	266	213	266	266
Adjusted R^2	0.01	0.01	0.15	0.16	0.40	0.15	0.15
First stage instruments							
Distance to Bujumbura (log)						−6.605 [2.519]**	
Altitude (log)						−22.488 [9.388]**	
Hansen J, *p*-value						0.97	
Partial F						7.25	

Notes: Dependent variable: discount rate. Robust standard errors in brackets clustered at community level. Column 1: dependent variable is community average. Sampling weights are not included, and results are qualitatively identical. Column 5: household 1998 controls included. Column 6: excluded instruments of first stage reported only. Column 7 contains beta coefficients of column 6. Estimations in column 7 do not include clustered standard errors.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Tables 3–5 thus suggest that exposure to conflict is correlated positively with altruistic behavior, risk-seeking behavior, and impatience.[18] But correlation does not equal causation. To attenuate potential endogeneity and omitted variables concerns in Tables 3–5, we rerun our regressions using 2SLS for each of the three types of preferences, and use distance to Bujumbura and altitude as instruments for our conflict measures.[19] Results are reported in columns 6 and 7 in Tables 3 and 5, and columns 7 and 8 in Table 4. Predicted violence is significant at the 5 percent level (or better) in the social preferences and time preferences regressions (Tables 3 and 5), and at the 10 percent level in the analysis of risk preferences (Table 4). The values of the coefficients are larger than when using OLS.[20] Also, our identifying assumption is that the distance and altitude variables affect only the distribution of violence, and do not impact on preferences otherwise. This assumption is contestable. Distance to the capital could proxy for distance to markets, in which case preferences likely depend on distances to Bujumbura (see Henrich et al. 2001). However, direct effects of proximity to the capital are likely minimal as most farmers operate at subsistence level, selling goods at local markets only (via which goods make their way to the capital). Such local markets are nearby in all communities in our sample (never over a two-hour walk away), reducing concerns about a correlation between geography and preferences. Even export crops, such as coffee, are usually sold to local intermediates or washing stations. Econometrically, this is confirmed by the test statistics in the bottom panel of Tables 3–5, which indicate our excluded instruments are correlated with the conflict variable (see the high partial F values) and correctly excluded from our second stage regression (the p-value of the Hansen J statistic is well above 0.10).

Finally, to assess the magnitude of these effects, we report the coefficients of all significant variables in each of the Tables 3–5 after having standardized the explanatory variables such that they have a mean of zero and a standard deviation of one. These so-called beta coefficients are reported in column 7 in Tables 3 and 5, and in column 8 of Table 4. Clearly the impact of conflict dominates all other impacts. For example, the beta coefficient of violence in the social preference regression is 0.49, more than twice as large as the beta coefficients of the other variables in the same column.

[18] The interrelation between these behaviors is not the focus of this paper. However, when using our full sample of 288 observations, we find altruism and patience are negatively correlated ($r = -0.12, p < 0.04$). Evidence provided by the scarce literature on the correlation between prosociality and patience using observed behavior suggest that the correlation is weak. For example, Fehr and Leibbrandt (2008) find no correlation between patience and social preferences in their study of Brazilian fishermen. This is in line with our findings: we also find no correlation when restricting the sample to nonconflict villages ($r = -0.06, p < 0.56$).

[19] We have also tried using village-level violence as an instrument for household-level victimization. These results are qualitatively identical and available on request. Psychological literature suggests, however, that the effect of violence on behavior does not depend on direct experience with violence (Yehuda 2002). If true, the exclusion restriction of this approach would be invalid.

[20] The finding that the 2SLS point estimate is somewhat larger than the OLS estimate is common in cross-country studies, usually attributed to measurement error (biasing the OLS estimates towards zero). Alternatively, IV results may produce overestimates of the true effect if included instruments are positively correlated with omitted variables that have the same sign as the endogenous conflict variables.

TABLE 6—CONFLICT AND REAL WORLD BEHAVIOR AND DECISIONS

Dependent variable	Social capital	Share of cash crops in total production	Expenditures on farm improvements	Social subsample 1998 same gender households	Risk subsample 1998 same gender households	Time subsample 1998 same gender households
	OLS (1)	Tobit (2)	OLS (3)	2SLS (4)	2SLS (5)	2SLS (6)
Percentage dead in attacks	0.033 [0.016]**	0.008 [0.005]*	−23.75 [12.810]*	3.080 [1.294]**	0.0930 [0.0574]*	0.424 [1.246]
Respondent is literate	0.374 [0.150]**	0.041 [0.039]	113.8 [92.48]	12.30 [4.625]***	−0.0957 [0.190]	−9.317 [5.915]
Respondent age	−0.001 [0.004]	0.002 [0.001]	−1.019 [3.598]	−0.346 [0.160]**	0.00154 [0.00657]	−0.249 [0.212]
Respondent is male	−0.111 [0.153]	0.079 [0.039]**	−35.47 [78.75]	−1.625 [5.255]	−0.267 [0.215]	7.504 [6.984]
Total land holdings per capita	0.027 [0.015]*	0.011 [0.004]***	−7.243 [9.346]	0.935 [0.309]***	−0.0304 [0.0197]	0.158 [0.428]
Land Gini coefficient	0.097 [0.353]	−0.465 [0.165]***	261.6 [291.7]	−16.85 [16.67]	−0.871 [0.996]	−57.04 [21.62]***
Distance to market	−0.092 [0.107]	−0.003 [0.043]	79.32 [54.88]	−8.053 [3.520]**	0.0952 [0.167]	6.209 [4.456]
Conflict over land	−0.897 [0.445]*	−0.530 [0.180]***	656.0 [464.9]	−33.90 [19.73]*	0.149 [1.318]	−40.05 [23.88]*
Ethnic homogeneity	0.003 [0.007]	0.002 [0.002]	−4.985 [3.597]	0.165 [0.148]	0.000822 [0.00725]	−0.599 [0.210]***
Socioeconomic homogeneity	0.044 [0.124]	0.011 [0.038]	39.08 [53.49]	−1.354 [2.698]	0.130 [0.158]	7.491 [5.159]
Population density	0.014 [0.186]	0.304 [0.103]***	−62.60 [159.5]	8.192 [6.769]	0.536 [0.374]	15.22 [8.582]*
Per capita total expenditure	−0.271 [0.139]*	0.099 [0.068]	113.5 [78.08]	1.172 [6.197]	−0.331 [0.254]	5.042 [5.455]
Constant	2.180 [1.863]	−3.094 [0.670]***	−635.3 [1261.1]	−3.600 [58.28]	1.793 [2.643]	−45.52 [74.99]
FE	Yes	Yes	Yes	Yes	Yes	Yes
N	274	277	279	212	158	205
Adjusted R^2		0.09	0.06	0.14	0.18	0.23
Hansen J, *p*-value				0.60	0.14	0.75
Partial F				6.09	5.07	5.99

Notes: Robust standard errors in brackets are clustered at community level. Sampling weights are not included, and results are qualitatively identical. Column 2 contains an unconditional Tobit regression with fixed effects, potentially inducing some bias in our estimate; regression using random effects is qualitatively similar. Columns 4–6: first stage instruments distance to Bujumbura (log) and altitude (log), results not shown.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

B. Robustness

We now report the outcomes of a series of robustness analyses. First, we explore whether differences in experimental play translate into systematic differences in behavior in "real life." We estimate several models and summarize our findings in Tables 6 and 7. In Table 6 we replace our experimental variables with survey-based

social-, risk-, and time-preference proxies. In column (1) the dependent variable is a social capital index in the spirit of Narayan and Pritchett (1999). This index comprises a weighted scale of respondents' participation in community organizations and the degree of membership in 2007. Consistent with our experimental variables, we find a positive correlation with exposure to violence. In addition, we find that many of the significant explanatory variables in this regression also showed up significantly in the regressions in Table 3. Literacy and per capita land holdings contribute positively to our measure of social capital, while conflict over land reduces it. Next, if conflict alters risk preferences we would expect an effect on investments and asset portfolio choice—skewing resources to more risky and profitable activities such as the production of cash crops (see Dercon 1996). In column 2 we find that households in regions exposed to greater levels of violence cultivate relatively more cash crops. (Recall that earlier we demonstrated that growing cash crops in 1998 did not invite subsequent conflict; see Table 2 columns 4–6.) Again this result is consistent with the experimental evidence. Some of the other control variables show up significantly too (in contrast to Table 4). Portfolio choice of crops is more risky as population density increases, conflict over land decreases, and the distribution of land holdings is more equal. In addition, male respondents and those with larger land holdings tend to invest more in risky cash crops. Lastly, in column 3 we use a measure of long-term investments—the share of expenditures on farm improvements in 2007—as our dependent variable. The assumption is that a greater share of durable investments reflects greater patience. Again we find our experimental results reflected in the survey data: households affected by greater levels of conflict invest less in their farms.

Next, to further assess the robustness of our findings we return to the potential bias introduced by nonrandom attrition into our sample. We follow the approach of Bellows and Miguel (2009) and probe the robustness of our findings for a subsample of respondents. This enables us to assess the possibility that our findings are due to changes in the composition of the population (rather than changing behavior of individual respondents). Specifically, Table 2, column 6 showed that men may be underrepresented in our postconflict sample (some 40 percent of households were female-headed in 2009). Hence, correlation between gender and preferences may bias our estimates in Tables 3–5. In columns 4–6 of Table 6 we reestimate our 2SLS models on a restricted sample of *same gender* respondents present in both 1998 and 2009—households where the gender of the household head did not change. We find that for social and risk preferences the results go through as before, with only minimal differences in the coefficients.

As a final robustness test we assess whether other types of shocks, such as natural disasters (drought and excess rainfall), plant diseases, and expenses on wedding ceremonies, etc., affect behavior in a similar fashion as conflict. We inserted, one by one, these nonconflict shocks as explanatory variables in the regression models in column 3 of Tables 3–5—while omitting the conflict measure. In Table 7 we present the coefficients on these nonconflict shocks. Interestingly, we now find few significant effects of such shocks on behavior in the experiments. Natural disasters and diseases do not produce the same traumatic responses as exposure to conflict—attenuating the risk that our results in Tables 3–5 are due to omitted variable effects. The one exception is a correlation between severe draughts and social preferences, suggesting that responses to economic shocks differ from conflict shocks.

Table 7—Preferences and Nonviolent Events

	OLS (1)	OLS (2)	OLS (3)
Dependent variable	Social	Risk	Time
Severe draught	11.644	−0.019	2.905
	[2.965]***	[0.204]	[4.160]
Excess rain	−1.674	−0.039	−1.183
	[4.325]	[0.213]	[7.214]
Manioc crop disease	9.451	0.156	5.653
	[6.464]	[0.265]	[7.145]
Upcoming ceremony			2.894
			[5.967]

Notes: Table summarizes coefficients of separate regressions including same controls and fixed effects as used in Tables 3–5. Robust standard errors in brackets, clustered at community level. Sampling weights are not included, and results are qualitatively identical.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

C. Behavior and Preferences

The evidence documented in the various tables is consistent with the idea that preferences of people are endogenous and respond to experiences or (changes in) the context. The nature of our data, however, does not permit us to rule out alternative explanations. For example, there may be selection on unobservables: i.e., communities with greater ethnic or political cleavages may be easier targets because they are less able to defend themselves or, conversely, communities with fewer cleavages may be more likely to be targeted because of their potential support "for the other side." The latter story would imply a correlation between exposure to violence and social preferences, such as we find in our data, but of course not a causal effect. While we cannot rule out such hypotheses entirely, there is nothing in our data to support them either. For example, when we include proxies for such cleavages (such as "the percentage of votes in favor of president Ndadaye" and "ethnic homogeneity") we find they are not statistically significant and do not affect our coefficients of interest.[21]

Alternatively, behavioral differences may be due to learning effects—learning about own preferences or those of others, or about consequences of behaviors over a range of contexts. Or, if conflict affects the social context which in turn affects behavior in "real life," then our experiment may also pick up such effects (see, e.g., Herrmann,

[21] Similarly, and following Blattman (2009), we have experimented with alternative proxies and controls to probe the robustness of our findings. (Results not shown but available on request.) For example, it may be that NGOs have selectively targeted high-conflict communities and have promoted prosocial preferences there. However, controlling for NGO interventions does not change our results. Similarly, selective exposure to post-conflict radio shows emphasizing reconciliation may potentially bias our results, but controlling for radio ownership does not affect our results. We also tested whether conflict induced sorting by people migrating to regions where their ethnic group was overrepresented. Our coefficient of interest is not affected when we control for changes in the ethnic composition of villages ("sorting") between 1993 and 2009 (for our subsample of 35 communities for which we have been able to collect this information), and population size. This is important because sorting could be another mechanism to explain why conflict victims behave more prosocially towards their peers.

Thöni, and Gächter 2008 for evidence on the impact of context and culture on cooperation and punishment in public good experiments). Our quasi-experimental approach implies imperfect control, and distinguishing cleanly preference shifts from learning effects is not possible. The benefit of our approach is that we are able to analyze the response to an event of first-order salience. Analysts routinely trade off control versus relevance when conducting experiments (e.g., List 2007). If we are to speculate about the underlying mechanism, however, we believe our data do not generally support the "learning interpretation." There is no reason to assume that a priori (i.e., without exposure to conflict) respondents' uninformed guesses about their true preferences would be biased systematically towards one direction or another, and also the variance in behavior is not smaller among victims than among nonvictims (and hence behavior does not really "converge" towards the new behavioral pattern either). Details of the formal tests are available on request.

Similarly, our data do not provide strong support for the alternative hypothesis that changes in behavior are due to conflict-induced changes in the social context (or structure). Contextual differences in violence and nonviolence communities are captured (at least to some extent) by the community controls in Tables 3–5 (such as ethnic and socioeconomic homogeneity, land inequality, and income), and our measures of violence still show up significantly. It may also be the case that villagers in affected communities expect more violence in the future, so that their behavior in the experiments reflects *expectations* of future exposure to violence rather than exposure to conflict in the past. Our data allow us to test this idea too. In our 2007 survey we included questions about subjective security in the postwar era, and we find no evidence that victims are more or less optimistic about their security situation than nonvictims.[22]

This brings us to the possibility that exposure to violence affects preferences. The economic literature suggests a mechanism for endogenous preferences: the neoclassical model based on optimizing agents pioneered by Becker and Mulligan (1997).[23] In their theory of the short-term adaptation of preferences, individuals can choose (at some cost) to increase their discount factor above their so-called "endowed level." For example, when expected future payoffs increase, the return to investments in a stock of "future-oriented capital" (i.e., raising the discount factor) goes up as well. Hence, Becker and Mulligan (1997) predict people make an effort to change their preferences and become more patient, thus increasing the level of their (net present value of) utility. Similar reasoning may be applied to endogenize social preferences and risk preferences. Individuals can (at some cost) deviate from their endowed levels of altruism and risk aversion. Altruism is typically modeled as other agents' welfare levels being arguments in the decision maker's utility function, with positive weights. Following the argument of Becker and Mulligan (1997), when an

[22] Results available on request.

[23] Alternatively, preference shifts may have a neurobiological basis. Even though the genetic code of individuals is fixed at birth, it is possible that trauma has long-lasting effects on behavior by influencing the expression of genes known to affect brain chemicals implicated in social behavior—a process called methylisation (cf. van IJzendoorn et al. 2010). For example, trauma may influence the expression of the gene regulating transportation of serotonin (5-HT) (Caspi et al. 2003), which has been linked to prosocial behavior (e.g., Crockett et al. 2008), discounting (Schweighofer et al. 2008), and risk taking (Kuhnen and Chiao 2009). Similarly, Kosfeld et al. (2005) show that altruism is regulated by the hormone oxytocin (OT), and de Dreu et al. (2010) find that OT not only influences within-group trust, but also stimulates aggression against competing "outsiders"—and hence OT may be the driver of parochial altruism (cf. Choi and Bowles 2007). Whether methylisation is the underlying mechanism linking shocks to preference shifts is an intriguing avenue for future research.

individual's peers do relatively well, the rational response for that individual would be to *increase* the weight she attaches to their welfare levels, and hence become *more* altruistic—because this raises the value of her own utility, too. This theoretical prediction seems at odds with the reality of conflict. A conflict shock leaves fellow villagers worse off, so their well-being should receive *less* weight in the utility function of a rational individual, rather than more (as documented in the experiments).[24]

Our findings appear not to be unique to the case of Burundi. Related literature has pointed to personal growth after trauma. In the political science literature, for example, Blattman (2009) notes that individuals abducted by the Lord's Resistance Army (in Uganda) are more politically active, and speculates this is due to "changes in personal goals, perspectives or self-regard" (p. 243; see also Bellows and Miguel 2009). In the psychological literature, there is ample discussion (with varying degrees of rigor) of how shocks can permanently alter someone's outlook on life (or the value of social networks; see Tedeschi and Calhoun 2004).

Closer to our paper, Gilligan, Pasquale, and Samii (2011) use behavioral experiments to document how exposure to conflict during Nepal's civil war contributes to a greater propensity to invest in trust-based transactions and to contribute to the public good. Similarly, Bauer et al. (2012) run a series of sharing experiments with a sample of children and adults in Georgia and Sierra Leone. They look at both inter- and intragroup sharing, and find partially supporting evidence for the theory that conflict breeds parochial altruism (as in Choi and Bowles 2007). We believe the results about intragroup altruism nicely complement our results. Together, they suggest, violence affects the behavior of both kids and adults, and the effects are both immediate and persistent.

VI. Conclusions

The literature on the consequences of civil wars has often emphasized its detrimental effects on households' ability to cope. According to this view, such civil wars may invite poverty traps. This pessimistic view on development has come under new scrutiny, however, from a few recent careful micro-level studies suggesting that exposure to conflict is not necessarily detrimental for development and may contribute to social capital (see Bellows and Miguel 2009; Blattman 2009; Gilligan, Pasquale, and Samii 2011; Bauer et al. 2012). Yet, social preferences are only one of a set of preferences of interest to development economists, and possibly affected by conflict shocks. We aim to extend earlier work by (i) including risk and time preferences in our analysis, and (ii) gauging such preferences with a series of incentive-compatible field experiments (rather than via a survey approach).

In this paper we set out to investigate the impact of conflict on social, risks, and time preferences and use data from a series of economic experiments using 300 respondents in 35 randomly selected communities in Burundi. We find that conflict is robustly correlated with behavior. Econometric analysis reveals that individuals in communities that were exposed to greater levels of violence display more altruistic

[24] Note that our results also extend to households who themselves have not been exposed to conflict, but saw the negative impact on their fellow villagers. These individuals, too, behave more prosocially, which is inconsistent with a theory predicting that people should care more about their peers when their peers are relatively well-off.

behavior to their neighbors, are more risk-seeking, and have higher discount rates. While our data do not allow us to exactly identify the mechanism linking conflict shocks to behavioral change, we discuss four candidate explanations: selection effects; changes in beliefs; social structure; and preferences. Future research could be designed to distinguish between these hypotheses so as to identify the mechanism linking trauma to behavioral change.

A key finding of this paper is that large temporary shocks may have long-term consequences: civil war violence that occurred between 1993 and 2003 has a clear impact on individual behavior in 2009. These consequences may even prove to be permanent if they invite preference shifts. Our evidence for Burundi suggests that the net effect on development is unclear. While exposure to violence encourages risk-taking and increases the weight people attach to their fellow community members' welfare, arguably positive features for development (at least within certain bounds), it also seems to trigger impatience. As impatience discourages savings, exposure to violence could also drag down investment levels in the presence of imperfect capital markets (as obviously prevail in Burundi). If so, the net effect on the ability of communities to rebound after conflict is ambiguous. Nevertheless, the results may partially explain the pattern of recovery observed in many postconflict settings, and thereby provide new evidence against pessimistic views on the destructive legacies of civil war.

REFERENCES

Altonji, Joseph G., Todd E. Elder, and Christopher R. Taber. 2005. "Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools." *Journal of Political Economy* 113(1): 151–84.

André, Catherine, and Jean-Philippe Platteau. 1998. "Land Relations under Unbearable Stress: Rwanda Caught in the Malthusian Trap." *Journal of Economic Behavior and Organization* 34(1): 1–47.

Andreoni, James, and Charles Sprenger. Forthcoming. "Risk Preferences are Not Time Preferences." *American Economic Review*.

Bauer, Michal, Alessandra Cassar, Julie Chytilová and Joseph Henrich. 2012. "Warfare Experience During Ontogeny Increases Egalitarian and Parochial Motivations." Unpublished.

Becker, Gary S., and Casey B. Mulligan. 1997. "The Endogenous Determination of Time Preference." *Quarterly Journal of Economics* 112(3): 729–58.

Bellows, John, and Edward Miguel. 2009. "War and Local Collective Action in Sierra Leone." *Journal of Public Economics* 93(11-12): 1144–57.

Blattman, Christopher. 2009. "From Violence to Voting: War and Political Participation in Uganda." *American Political Science Review* 103(2): 231–47.

Brakman, Steven, Harry Garretsen, and Marc Schramm. 2004. "The Spatial Distribution of Wages: Estimating the Helpman-Hanson Model for Germany." *Journal of Regional Science* 44(3): 437–66.

Bundervoet, Tom, Philip Verwimp, and Richard Akresh. 2009. "Health and Civil War in Rural Burundi." *Journal of Human Resources* 44(2): 536–63.

Carmil, Devora, and Shlomo Breznitz. 1991. "Personal Trauma and World View – Are Extremely Stressful Experiences Related to Political Attitudes, Religious Beliefs, and Future Orientation?" *Journal of Traumatic Stress* 4(3): 393–405.

Caspi, Avshalom et al. 2003. "Influence of Life Stress on Depression: Moderation by a Polymorphism in the 5-HTT Gene." *Science* 301(5631): 386–89.

Chrétien, Jean-Pierre, and Melchior Mukuri. 2000. *Burundi, la Fracture Identitaire. Logiques de Violence et Certitudes Ethniques, (1993–1996)*. Paris: Karthala.

Cerra, Valerie, and Sweta Chaman Saxena. 2008. "Growth Dynamics: The Myth of Economic Recovery." *American Economic Review* 98(1): 439–57.

Chen, Siyan, Norman V. Loayza, and Marta Reynal-Querol. 2008. "The Aftermath of Civil War." *World Bank Economic Review* 22(1): 63–85.

Choi, Jung-Kyoo, and Samuel Bowles. 2007. "The Coevolution of Parochial Altruism and War." *Science* 318(5850): 636–40.

Collier, Paul, V. L. Elliott, Håvard Hegre, Anke Hoeffler, Marta Reynal-Querol, and Nicholas Sambanis. 2003. *Breaking the Conflict Trap: Civil War and Development Policy*. New York: Oxford University Press.

Cramer, Christopher. 2006. *Civil War Is Not a Stupid Thing: Accounting for Violence in Developing Countries*. London: Hurst & Company.

Crockett, Molly J., Luke Clark, Golnaz Tabibnia, Matthew D. Lieberman, and Trevor W. Robbins. 2008. "Serotonin Modulates Behavioral Reactions to Unfairness." *Science* 320(5884): 1739.

Davis, Donald R., and David E. Weinstein. 2002. "Bones, Bombs, and Break Points: The Geography of Economic Activity." *American Economic Review* 92(5): 1269–89.

De Dreu, Carsten K. W., Lindred L. Greer, Michel J. J. Handgraaf, Shaul Shalvi, Gerben A. Van Kleef, Matthijs Baas, Femke S. Ten Velden, Eric Van Dijk, and Sander W. W. Feith. 2010. "The Neuropeptide Oxytocin Regulates Parochial Altruism in Intergroup Conflict Among Humans." *Science* 328(5984): 1408–11.

Dercon, Stefan. 1996. "Risk, Crop Choice, and Savings: Evidence from Tanzania." *Economic Development and Cultural Change* 44(3): 485–513.

Faul, Franz, Edgar Erdfelder, Albert G. Lang, and Axel Buchner. 2007. "G*Power 3: A Flexible Statistical Power Analysis Program for the Social, Behavioral, and Biomedical Sciences." *Behavior Research Methods* 39(2): 175–91.

Fehr, Ernst, and Andreas Leibbrandt. 2008. "Cooperativeness and Impatience in the Tragedy of the Commons." Institute for the Study of Labor (IZA) Discussion Paper 3625.

Fitzgerald, John, Peter Gottschalk, and Robert Moffitt. 1998. "An Analysis of Sample Attrition in Panel Data: The Michigan Panel Study of Income Dynamics." National Bureau of Economic Research Working Paper 220.

Gilligan, Michael, Benjamin Pasquale, and Cyrus Samii. 2011. "Civil War and Social Capital: Behavioral Game Evidence from Nepal." Unpublished.

Harbaugh, William T., Kate Krause, and Lise Vesterlund. 2002. "Risk Attitudes of Children and Adults: Choices over Small and Large Probability Gains and Losses." *Experimental Economics* 5(1): 53–84.

Henrich, Joseph, Robert Boyd, Samuel Bowles, Colin Camerer, Ernst Fehr, Herbert Gintis, and Richard McElreath. 2001. "In Search of Homo Economicus: Behavioral Experiments in 15 Small-Scale Societies." *American Economic Review* 91(2): 73–78.

Herrmann, Benedikt, Christian Thöni, and Simon Gächter. 2008. "Antisocial Punishment Across Societies." *Science* 319(5868): 1362–67.

Human Rights Watch. 1998. *Proxy Targets: Civilians in the War in Burundi*. New York: Human Rights Watch.

Kahneman, Daniel, and Amos Tversky. 1979. "Prospect Theory: An Analysis of Decision under Risk." *Econometrica* 47(2): 263–91.

Kalyvas, Stathis N. 2006. *The Logic of Violence and Civil War.* New York: Cambridge University Press.

Kosfeld, Michael, Markus Heinrichs, Paul J. Zak, Urs Fischbacher, and Ernst Fehr. 2005. "Oxytocin Increases Trust in Humans." *Nature* 435: 673–76.

Krueger, Robert, and Kathleen T. Krueger. 2007. *From Bloodshed to Hope in Burundi: Our Embassy Years during Genocide*. Austin: University of Texas Press.

Kuhnen, Camelia M., and Joan Y. Chiao. 2009. "Genetic Determinants of Financial Risk Taking." *PLoS ONE* 4(2): e4362.

List, John A. 2007. "Field Experiments: A Bridge Between Lab and Naturally-Occurring Data." *Advances in Economic Analysis & Policy* 6(2): Article 8.

Liebrand, Wim B. G. 1984. "The Effect of Social Motives, Communication and Group Size on Behavior in an N-Person Multi Stage Mixed-Motive Game." *European Journal of Social Psychology* 14(3): 239–64.

Miguel, Edward, and Gerard Roland. 2011. "The Long-Run Impact of Bombing Vietnam." *Journal of Development Economics* 96(1): 1–15.

Narayan, Deepa, and Lant Pritchett. 1999. "Cents and Sociability: Household Income and Social Capital in Rural Tanzania." *Economic Development and Cultural Change* 47(4): 871–97.

Netzer, Nick. 2009. "Evolution of Time Preferences and Attitudes toward Risk." *American Economic Review* 99(3): 937–55.

Ngaruko, Floribert, and Janvier D. Nkurunziza. 2000. "An Economic Interpretation of Conflict in Burundi." *Journal of African Economies* 9(3): 370–409.

Punamäki, Raija-Leena, Samir Qouta, and Eyad El Sarraj. 1997. "Relationships between Traumatic Events, Children's Gender, and Political Activity, and Perceptions of Parenting Styles." *International Journal of Behavioral Development* 21(1): 91–109.

Raleigh, Clionadh, Andrew Linke, Havard Hegre, and Joakim Karlsen. 2010. "Introducing ACLED: An Armed Conflict Location and Event." *Journal of Peace Research* 47(5): 651–60.

Raleigh, Clionadh, Andrew Linke, Havard Hegre, and Joackim Karlsen. 2010. "Introducing ACLED: An Armed Conflict Location and Event: Dataset." *Journal of Peace Research*, 47(5): 1–10 (accessed July 2009).

Schweighofer, Nicolas, Mathieu Bertin, Kazuhiro Shishida, Yasumasa Okamoto, Saori C. Tanaka, Shigeto Yamawaki, and Kenji Doya. 2008. "Low-Serotonin Levels Increase Delayed Reward Discounting in Humans." *Journal of Neuroscience* 28(17): 4528–32.

Tanaka, Tomomi, Colin F. Camerer, and Quang Nguyen. 2010. "Risk and Time Preferences: Linking Experimental and Household Survey Data from Vietnam." *American Economic Review* 100(1): 557–71.

Tedeschi, Richard G., and Lawrence G. Calhoun. 2004. "Posttraumatic Growth: Conceptual Foundations and Empirical Evidence." *Psychological Inquiry* 15(1): 1–18.

United Nations. 1996. *Report of the Secretary-General on the Situation in Burundi*. New York: United Nations.

United Nations Population Fund. 2002. Enquête Socio-Démographique, Santé et Reproduction 2002 (accessed July 2009).

Uvin, Peter. 1999. "Ethnicity and Power in Burundi and Rwanda: Different Paths to Mass Violence." *Comparative Politics* 31(3): 253–71.

van IJzendoorn, Marinus H., Kristin Caspers, Marian J. Bakermans-Kranenburg, Steven R. H. Beach, and Robert Philibert. 2010. "Methylation matters: Interaction Between Methylation Density and Serotonin Transporter Genotype Predicts Unresolved Loss or Trauma." *Biological Psychiatry* 68(5): 405–7.

Voors, Maarten, Eleonora E.M. Nillesen, Philip Verwimp, Erwin H. Bulte, Robert Lensink, and Daan van Soest. 2007. Burundi Priority Household Survey 2007 .

Voors, Maarten, Eleonora E.M. Nillesen, Philip Verwimp, Erwin H. Bulte, Robert Lensink, and Daan van Soest. 2009. Burundi Experiments Exit Survey.

Voors, Maarten J., Eleonora E. M. Nillesen, Philip Verwimp, Robert Lensink, Erwin H. Bulte, and Daan P. Van Soest. 2012. "Violent Conflict and Behavior: A Field Experiment in Burundi: Dataset." *American Economic Review.* http://dx.doi.org/10.1257/aer.102.2.941.

Yehuda, Rachel. 2002. "Post-Traumatic Stress Disorder." *New England Journal of Medicine* 346(2): 108–114.

This article has been cited by:

1. Hannah Schildberg-Hörisch. 2018. Are Risk Preferences Stable?. *Journal of Economic Perspectives* **32**:2, 135-154. [Abstract] [View PDF article] [PDF with links]
2. Chie Hanaoka, Hitoshi Shigeoka, Yasutora Watanabe. 2018. Do Risk Preferences Change? Evidence from the Great East Japan Earthquake. *American Economic Journal: Applied Economics* **10**:2, 298-330. [Abstract] [View PDF article] [PDF with links]
3. Moses Shayo, Asaf Zussman. 2017. Conflict and the Persistence of Ethnic Bias. *American Economic Journal: Applied Economics* **9**:4, 137-165. [Abstract] [View PDF article] [PDF with links]
4. Michal Bauer, Christopher Blattman, Julie Chytilová, Joseph Henrich, Edward Miguel, Tamar Mitts. 2016. Can War Foster Cooperation?. *Journal of Economic Perspectives* **30**:3, 249-274. [Abstract] [View PDF article] [PDF with links]
5. Alberto Alesina, Paola Giuliano. 2015. Culture and Institutions. *Journal of Economic Literature* **53**:4, 898-944. [Abstract] [View PDF article] [PDF with links]
6. Stefano DellaVigna, Ruben Enikolopov, Vera Mironova, Maria Petrova, Ekaterina Zhuravskaya. 2014. Cross-Border Media and Nationalism: Evidence from Serbian Radio in Croatia. *American Economic Journal: Applied Economics* **6**:3, 103-132. [Abstract] [View PDF article] [PDF with links]
7. Michael Callen, Mohammad Isaqzadeh, James D. Long, Charles Sprenger. 2014. Violence and Risk Preference: Experimental Evidence from Afghanistan. *American Economic Review* **104**:1, 123-148. [Abstract] [View PDF article] [PDF with links]